

**Comment on *Murex tubercularis* Montagu, 1803 (currently *Cerithiopsis tubercularis*; Mollusca, Gastropoda, CERITHIOPSIDAE): proposed conservation of usage of the specific name by designation of a neotype**  
(Case 3532; see BZN 68: 41–46; 205)

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In the present state of our knowledge, we do not support this application, and we raise doubts on the reasons brought to support it. In our opinion, the original type material (Exeter Museum) consists of two conspecific specimens, both referable to *Cerithiopsis barleei* Jeffreys, 1867 (therefore, the type designation by Marshall was correct); the 'probable syntype BMNH 20090384' would not help in identifying with certainty the species, since it belongs to a complex of cryptic species (named and unnamed), which in most of the cases can be diagnosed only by soft parts morphology and/or genetics.

1. Montagu's *Murex tubercularis*.

Montagu's (1803) description of *Murex tubercularis* fits at least 10 Recent European species of cerithiopsid. Montagu (1808) included an additional feature ('It has three series of tubercles of equal size on each volution, . . .'), which restricts the number of possible candidates but still does not allow a positive identification. However, the two specimens in Montagu's collection (Exeter Museum) do allow such identification since their identity is less equivocal than recently supposed. The first specimen is the lectotype of *M. tubercularis* designated by Marshall (1978, fig. 13C) and re-figured by Cecalupo & Robba (2010, figs. 1E-F, BZN 68: 43, figs. 1B–D). The existence of specimens lighter in colour (as the lectotype) was known to Montagu (1808): 'An elegant sub-pellucid, white variety of this shell was found near Dunbar, by Mr. LASKEY.' The protoconch (now partly broken) has been described by Marshall (1978, p. 83) as corresponding to published figures of *Cerithiopsis barleei* Jeffreys, 1867, and also its teleoconch features correspond to *C. barleei*, including the colour (whitish shells of this species are not rare). The lectotype certainly does not belong to *C. powelli* Marshall, 1978 as Cecalupo & Robba (2010; BZN 68: 42 March 2011) supposed, because this species clearly differs from *C. barleei*, not only in

protoconch sculpture but also in many teleoconch features. The second specimen (paralectotype) (it remained in Exeter and was never seen by Marshall), lacking the apex, has been figured by Cecalupo & Robba (2010, figs. 1C-D; BZN 68: 43, fig. 1A). It is ca. 6.5 mm long, with 9–10 teleoconch whorls, three rows of tubercles on all whorls, with an additional peripheral cord on the last whorl, and a smooth base (no basal cord). These features are perfectly concordant with those of *C. barleei*, and all characters of this specimen also fit the original description by Montagu. It is therefore easily argued that Montagu's concept of his *Murex tubercularis* was (at least originally) based on specimens of the species subsequently called *Cerithiopsis barleei* Jeffreys, 1867. Marshall's (1978) interpretation was thus correct and his designation of the only protoconch-bearing specimen as lectotype of *U. tubercularis* was justified.

2. The modern concept of *Cerithiopsis tubercularis*, and the type species of *Cerithiopsis*.

There is general agreement that the modern concept of *Cerithiopsis tubercularis* arose with Forbes & Hanley's (1850) introduction of the genus *Cerithiopsis*, nominally based on *Murex tubercularis* Montagu. The concept introduced by these two authors defines the type species of *Cerithiopsis* (i.e. *C. tubercularis* sensu Forbes & Hanley, 1851, nec Montagu) as a species characterized by: protoconch multispiral and smooth; teleoconch with three rows of tubercles; last whorl with 5 or 6 spirals (1 or 2 at the base); dimension ca. 6 mm. Forbes & Hanley's (1851, p. 364) description of their specimens includes also notes on the coloration of the head-foot: 'The general colour is white, with dusky markings; . . . The head and anterior half of the foot are dark.' (with a remark that 'Some specimens are flake white, except some sulphur-yellow points behind the eyes . . .'). According to our current knowledge (see below), this clearly indicates that they were mixing more species, of which at least one ('The head and anterior half of the foot are dark'; Forbes & Hanley, 1851) closely resembles the Croatian specimens figured by Prkić & Mariottini (2010) as '*C. tubercularis* (Montagu, 1803) sensu auct. nec Marshall (1978)'. This concept, underlying the use of the name '*Cerithiopsis tubercularis*' for specimens with a smooth protoconch, has been followed almost invariably by all subsequent authors. Among them, Jeffreys (1867) described the new species *C. barleei* comparing it with *C. tubercularis* [sensu Forbes & Hanley (1850–1851)], and found it to differ among other details by 'the upper whorls [protoconch] being finely striated in the line of the spire [axially]' (Jeffreys 1867, 269). For *C. tubercularis* Jeffreys (1867, 266) wrote: 'the first 4 or 5 whorls are quite smooth'. Remarkably, Jeffreys (1884) did not find specimens labelled '*Murex tubercularis*' during his study of Montagu's collection at Exeter. Apparently there was only a sample labelled '*Murex reticulatus*' that contained *C. tubercularis* (sensu Forbes & Hanley). Recently, Prkić & Mariottini (2010) have shown that more than one species correspond, with principal shell characters (apex and base), to the (erroneous) concept of *C. tubercularis* introduced by Forbes & Hanley (1850–1851). This complex comprises (at least in Croatia, northern Adriatic Sea) cryptic species, which can be diagnosed only by examining the soft parts morphology and are confirmed by ongoing genetic research (Modica, Mariottini, Prkić & Oliverio, unpublished), and other such morphotypes are known from the rest of the Mediterranean and the northeastern Atlantic.

### 3. Common usage and lectotype vs. neotype.

It is true that the prevailing usage of the name in the last 160 years has followed Forbes & Hanley's erroneous concept of *Cerithiopsis tubercularis*. Even after Marshall's lectotype designation, most authors have kept the old position, although Warén (1980) described *C. tubercularis* as a 'difficult species, the taxonomy of which still is unclear', and van Aartsen et al. (1984) used the denomination '*C. tubercularis* auct.', and noted that the lectotype 'most probably belongs to the species so far known as *C. barleei* Jeffreys, 1867', thereafter concluding: 'Nevertheless it seems that the most common European *Cerithiopsis* species, which has been known for many years as *C. tubercularis* (Montagu, 1803) should change its name, whereas the much more rare *C. barleei* should be called *C. tubercularis*.' The same opinion was expressed by P. Bouchet, who in 1986 saw both syntypes in EXEMS (pers. comm. between Bolton, Cecalupo and Robba). Admittedly, changes in the nomen usage would affect (from the purely nomenclatural point of view) the name of some common European species, one of which is the type species of the genus *Cerithiopsis*, which in turn is the type genus of the family CERITHIOPSIDAE.

However, we highlight again here that *Cerithiopsis tubercularis* sensu auctt. (non Montagu) actually consists of a complex of cryptic species, difficult or even impossible to identify by examining only the shell characters. The prevailing usage has in fact been to apply the name *Cerithiopsis tubercularis* to several different species, although it is mostly impossible (by shell characters only) to say which one in each case. Therefore, it would neither be possible to identify with certainty which species Forbes & Hanley (1851) intended when introducing the genus *Cerithiopsis*, nor what species the 'probable syntype BMNH 20090384' belongs to, although it clearly belongs to *Cerithiopsis* s. str. The prevailing usage of the name *C. tubercularis* has not been unequivocal, being referred to more than one species, and thus, suppressing Marshall's (1978) lectotype designation and designating the 'probable syntype BMNH 20090384' as a neotype would not contribute significantly to nomenclatural stability, but would simply move the problem to another group of named and unnamed species. In fact, this solution would simply maintain as valid *Cerithiopsis barleei* Jeffreys, 1867, leaving the correct identification of *C. tubercularis* to a future study.

4. We recommend here that the 'probable syntype BMNH 20090384' should NOT be designated as neotype of any taxon (obviously the indication of this specimen as neotype by Cecalupo & Robba (2010) was not intended as a designation, otherwise it would be invalid due to the concurrent existence of the lectotype), and that, before taking any decision, the intricate puzzle of the species complex of *C. tubercularis* sensu auctt. must be solved based on the study of live collected specimens, with types designated from specimens characterised on morphology and colour patterns of the living animals (even a perfect shell is not what is needed here) and genetics.

At that point, the following two alternative possibilities would be available, and we feel it would not be wise to choose now either of them at the present state of our knowledge:

A. The Commission does not suppress Marshall's (1978) designation, which was valid, correctly interpreting Montagu's original concept of *Murex tubercularis*. The name *tubercularis* Montagu, 1803 is placed on the Official List of Specific Names in Zoology as defined by the lectotype designated by Marshall (1978). This would mean

that *Cerithiopsis tubercularis* (Montagu, 1803) eventually becomes a senior synonym of *C. barleei* Jeffreys, 1867.

B. The Commission suppresses Marshall's (1978) designation, and a neotype is designated based on a morphologically and genetically characterised specimen. The name *tubercularis* Montagu, 1803 is placed on the Official List of Specific Names in Zoology as defined by the designated neotype. This procedure would maintain as valid *Cerithiopsis barleei* Jeffreys, 1867, and preserve the name of the type species of the genus *Cerithiopsis*, probably based on one of the species originally intended by the authors (Forbes & Hanley, 1850–1851).

### Additional references

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- Montagu, G. 1808. *Supplement to Testacea Britannica with additional plates*. v, 7–184 pp., 17–30 pls. S. Woolmer, Exeter.
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### Comment on the proposed resolution of homonymy between CHILODONTINAE Eigenmann, 1910 (Pisces, Characiformes) and CHILODONTINAE Wenz, 1938 (Mollusca, Gastropoda)

(Case 3555; see BZN 68: 175–179; 281–282)

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We wish to express our support for the modification of CHILODONTINAE Wenz, 1938 to CHILODONTAINAE to avoid homonymy. There is no name available to replace the gastropod family-group name which is in long and well-established usage and its retention in emended form is the best possible solution.

**Comment on the proposed establishment of availability of *Balintus* d'Abbrera, 2001, *Gulliveria* d'Abbrera & Bálint, 2001, *Salazaria* d'Abbrera & Bálint, 2001, *Megathecla* Robbins, 2002 and *Gullicaena* Bálint, 2002 (Insecta, Lepidoptera, LYCAENIDAE)**  
(Case 3458; see BZN 65: 188–193; 66: 271–272, 349–351; 68: 206–211)

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This is a response to the comments of Bálint & d'Abbrera (BZN 68: 206–210) and of Craig (BZN 68: 210–211). The core issue of Case 3458 is whether the eight generic names proposed by d'Abbrera and Bálint (d'Abbrera, 2001) satisfy Article 13.1 of the Code. In the original application and subsequent comment, we noted that the words in these generic descriptions differentiate the type species, not the genus. The characters differentiate the type species, not the genus. Since Article 13.1 was not satisfied, we proposed a solution in which all names that were in use were to be considered to be available and those that were not in use were to be considered to be unavailable. Evidence as to which names were in use was presented in the original application.

The purpose of this comment is to show (briefly using representative examples) that the recent comments on Case 3458 by Bálint & d'Abbrera are not relevant to the core issue of Case 3458 and that the recent comments on Case 3458 by Craig are not accurate.

Paragraphs 1–7 and 10–11 in the comments of Bálint & d'Abbrera make the case that these authors intended to make their generic names available. No one has argued otherwise, so far as we are aware, but it is not relevant because intent, by itself, does not satisfy Article 13.1.

Paragraph 8 in the comments of Bálint & d'Abbrera makes the point that four generic names that were published in 1973 by Eliot were worded similarly to the descriptions of d'Abbrera and Bálint, but have been considered to be available. In response, the 1973 generic names were made available under the Second Edition of the Code (1964), which had a different wording from the Fourth Edition of the Code (1999). Further, the wording of the 1973 descriptions is not identical to that used by d'Abbrera and Bálint and, in evident contrast to d'Abbrera and Bálint, Eliot provided generic differentiating characters for his new genera. For these reasons, each case needs to be decided on its own merits, and a conclusion in one case need not apply to the other.

Paragraph 9 in the comments of Bálint and d'Abbrera reinterprets their original verbal description to say that they distinguished the genus, not the type species. But this verbal reinterpretation is falsified by their proposed characters, which distinguish the type species, not the genus.

Paragraphs 12–16 & 18 in the comments of Bálint & d'Abbrera refer to a range of perceived ethical issues. The first response is that these comments are not relevant because the Code of Ethics states that 'the Commission is not empowered to

investigate or rule upon alleged breaches of [ethical principles]'. The second response is that we view the ethical issues raised by this case very differently than do Bálint and d'Abrera, as alluded to in an earlier comment, but these comments are not an appropriate forum to discuss ethical issues, as just noted. The third response is that the Preamble of the Code states that all Code provisions and recommendations, including the Code of Ethics, are subservient to the promotion of stability and universality. In each of our publications related to Case 3458, we have made clear that this shared core value is our guiding principle. The fourth response is that the Commission has considered three applications by Lamas during his term as a Commissioner. In every one of these cases Lamas has recused himself (abstained) from voting on the application.

Paragraphs 17 & 19 in the comments of Bálint & d'Abrera contain words like tumult, chaos, destabilising, manipulative, and agenda. We are at a loss to see how the arguments of which these 'emotive' words are a part are relevant or appropriate to a simple case of nomenclatural availability.

The recent comments of Craig are inaccurate. For example, Craig refers to d'Abrera and Bálint's 'eight new genera, that nobody has yet questioned with regard to the soundness of the concepts involved.' The lack of nomenclatural soundness is the reason for the original application in Case 3458. The lack of taxonomic soundness is the reason that the seven species placed in *Salazaria* d'Abrera and Bálint (in d'Abrera, 2001) were later treated as members of four different genera, as noted in the original application. Regardless of which taxonomy is correct, Craig's statement is not true.

Other recent comments by Craig make unsubstantiated allegations. As an example, 'For reasons that the applicants have never fully explained, the Commission is being asked to make five (supposedly unavailable) names available, two of which are to be immediately suppressed and thus made unavailable, along with six others, five of which have barely been discussed in the application and appear to represent taxa for which there would then be no alternative generic names.' We make no pretense of being able to understand this sentence, but in the original application we explained the reasons for our proposal and gave the evidence to support it. To be blunt, we are troubled that Craig accuses us of not fully explaining our proposal without specifying those issues that were not fully explained.

In short, the core issue of Case 3458 is whether the eight genera proposed by d'Abrera and Bálint in 2001 satisfied Article 13.1 of the Code. The recent comments of Bálint & d'Abrera and of Craig do not address this simple issue.

### **Comment on the proposed precedence of *Sematura* Dalman, 1825 over *Mania***

**Hübner, 1821 Lepidoptera, SEMATURIDAE**

(Case 3531; see BZN 68: 184–189)

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I believe it is not fair for any author to ignore the laws of priority. In this case, is it Huebner's fault that subsequent authors made mistakes or ignored his work?

The priority of *Mania* over *Sematura* was well established as far back as 1892, by Kirby, in the first comprehensive moth catalogue (Kirby, 1892 – an important reference overlooked in the proposal). Before that date *Sematura* had been used only a few times (the dates of both *Mania* and *Sematura* were also well clarified in Sherborn; therefore there is no reason to keep using the latter). Checking the references listed in the proposal it is clear that the majority of the authors are not taxonomists but mostly list compilers, who usually apply whichever names are currently in use.

Another argument used in the proposal is that *Sematura* was used to establish SEMATURIDAE. To me this is irrelevant as there are many cases in Lepidoptera, where family-group names are based on junior synonyms (EPIPASCHIINAE, PERICOPINAE, etc.).

Also, 'current use' is not an argument strong enough to justify this kind of action. The Commission itself has reestablished 'forgotten' names in cases much more important than this, as in resurrecting *Plutella xylostella* L. against *P. maculipennis* Z., a worldwide pest where hundreds of publications had used the latter name.

### Additional references

Kirby, W.F. 1892. *A synonymic catalogue of Lepidoptera Heterocera. (Moths). Vol. 1 Sphinges and bombyces.* xi, 951, 45 pp. Gurney & Jackson, London.

**Comment on the proposed conservation of *Crotalinus catenatus* Rafinesque, 1818 (currently *Sistrurus catenatus*) and *Crotalus tergeminus* (currently *Sistrurus tergeminus*; Reptilia, Serpentes) by designation of neotypes for both species (Case 3571: see BZN 68: 271–274)**

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In our case we proposed the designation of USNM 86472 at the National Museum of Natural History, U.S.A., from Winfield, Cowley, Kansas, U.S.A. as the neotype of *Crotalus tergeminus*, the Western Massassagua. That decision was based on the lack of agreement by previous authors as to the provenance of the two syntypes of the nominal species variously cited to be from between the Mississippi River and the Rocky Mountains headwaters of the Arkansas River, from what is now western Iowa

or from northeastern Colorado (Dundee, '1996', 1997, p. 8). Stephen Mackessy of the University of Northern Colorado (personal communication) recently informed us that *Sistrurus tergeminus* is unknown from northeastern Colorado. However, the taxon still occurs in southwestern Iowa (Christiansen & Fiesemann, 1993). It now seems evident that the syntypes of this form were collected during a side trip up the Boyer River (now in Harrison County, Iowa) from the Missouri River where Say (in James, 1822, pp. 45–46) states 'we saw numbers of the smaller species of rattle snake'. Fortunately, we now have been able to locate well-preserved examples of this taxon from western Iowa. In the light of this information we ask the International Commission on Zoological Nomenclature to set aside our request to designate USNM 86472 as the neotype of *Crotalus tergeminus*. We ask instead that the Commission act to designate specimen at Drake University DU 3917 from 4.5 miles north of Hastings, Willow Slough, Mills County, Iowa, U.S.A. as the neotype of *Crotalus tergeminus* Say in James (1822, p. 499). Note that the Mills County snake is from a locale approximately 40 miles south of Harrison County, Iowa. The proposed neotype is a *Sistrurus* 715 mm in total length, having 157 ventrals, 28 subcaudals, a dorsal pattern of 42 dark brown blotches that are in marked contrast to the lighter ground color and the venter light with numerous darker markings.

We take this opportunity to clarify our request that USNM 526 be designated the neotype of *Crotalinus catenatus* Rafinesque 1818, p. 41. This snake may be the holotype (Adler, 1963) of *Crotalus messasaugus* Kirtland that is the next available name for the Eastern Massasauga, in the event that our request to establish a neotype for *Crotalinus catenatus* is denied. Adler (1963) has provided a detailed description of USNM 526 whose salient characters are: 830 mm in total length, 138 ventrals, 19 subcaudals, dorsum very dark with blotches not strongly contrasting with ground color, and venter black with some light markings.

In consequence of the above we request the International Commission to substitute the following in place of 8(2) in our original application when considering Case 3571:

(2) to use its specific powers to designate specimen Drake University DU 3917 from Mills County, Iowa as the neotype of *Crotalus tergeminus* Say in James, 1822;

#### **Additional reference**

Christiansen, J. & Fiesemann, J. 1993. Massasauga rattlesnake bites in Iowa. *Iowa Medicine*, 83(5): 187–191.

#### **Comment on *Stegosaurus* Marsh, 1877 (Dinosauria, Ornithischia): proposed replacement of the type species with *Stegosaurus stenops* Marsh, 1887**

(Case 3536; see BZN 68: 127–133; 213–217)

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I wish to supplement my previous comment (BZN 68: 215–217) on Case 3536, with respect to putative individual variation reported by Maidment et al. (2008) for *Stegosaurus armatus*.



Carpenter (BZN 68: 215) notes that the range of variation within *Stegosaurus armatus* (sensu Maidment et al.) is suspect because it cannot be replicated in other large stegosaur samples (e.g. *Kentrosaurus aethiopicus*). However, it should be noted that most stegosaur taxa apart from *Stegosaurus ungulatus* and *S. stenops* (sensu Carpenter & Galton, 2001) and *Kentrosaurus* (e.g. *Loricatosaurus*, *Chungkingosaurus*, etc.) are known only from holotypes or a few specimens (see Appendix 1 in Maidment et al.). For example, Barden & Maidment (2011) indicated that variation within specimens of *Kentrosaurus aethiopicus* from the Tendaguru Formation of Tanzania may be attributable to sexual dimorphism. Moreover, recent histological analysis of dermal armour in *Stegosaurus* (Hayashi et al., 2012) suggests that the morphology of dermal armor in stegosaurs can be influenced by ontogenetic factors (e.g. immaturity, old age). Therefore, Maidment et al. appear to be correct in interpreting variation within *Stegosaurus* as either ontogenetic or sexually dimorphic. For example, femur lengths for *Stegosaurus stenops* specimens DMNH 1483 and DMNH 2818 are given by Hayashi et al. as 950 mm and 1,048 mm respectively, while the femur of the holotype of *Stegosaurus stenops* (USNM 4934) is 1080 mm and the femur of *S. ungulatus* specimen USNM 6646 measures 1200 mm (Lull, 1921, p. 117). By comparison, the femora of YPM 1853 (lectotype of *Stegosaurus ungulatus*) and YPM 1858 are 1348 mm long (Carrano, 2006), while Galton (2001) reported the femoral length for USNM 4936 (referred by him to *Stegosaurus stenops*) as 1190 mm. Therefore, it seems parsimonious to conclude that DMNH 1483, DMNH 2818, USNM 4934, USNM 4936, USNM 6646, YPM 1853, and YPM 1858 are progressive ontogenetic stages of the same species, given the locality data for these specimens in Turner & Peterson (1999).

Although the holotype of *S. stenops* is by far the most complete *Stegosaurus* specimen, it should also be noted that *S. ungulatus*, although based on less material than USNM 4934, is also based on material of taxonomic utility to be compared against other stegosaur genera. If *Stegosaurus stenops* is designated the type species of *Stegosaurus*, then *S. ungulatus* would either become a junior subjective synonym of *S. stenops* or a referred species of *Stegosaurus*. Therefore, while I reaffirm my support for Case 3536, I also urge the Commission to address the priority of *S. ungulatus* over *S. stenops* if the proposals in Case 3536 are approved by a final decision.

### Additional references

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**Comment on the proposed conservation of usage of *Mastodon waringi* Holland, 1920 (currently *Haplomastodon waringi*; Mammalia, Proboscidea) by designation of a neotype**

(Case 3480; see BZN 66: 164–167, 358–359; 67: 96, 181–182, 333; 68: 80–81)

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Alberdi et al. (BZN 68: 80–81) oppose designating a diagnostic neotype for *Mastodon waringi*, the oldest valid species of *Haplomastodon*, in order to stabilize the name *Haplomastodon* (see Lucas, BZN 66: 164–167). They also oppose recognition of the remaining portions of the holotype of *Mastodon chimborazi*, which is the type species of *Haplomastodon* by original designation, as a means to stabilize *Haplomastodon* (see Ferretti, BZN 66: 358). The opposition to these steps by Alberdi et al. is based on their opinion that *Haplomastodon* Hoffstetter, 1950 is a junior subjective synonym of *Stegomastodon* Pohlig, 1912 (e.g. Prado et al., 2005). However, there is growing opposition to this subjective taxonomic judgment by those who regard *Haplomastodon* as either a distinct genus or as a junior subjective synonym of *Notiomastodon* Cabrera, 1929 (e. g., Ferretti, 2008, 2010; Lucas & Alvarado, 2010; Lucas et al., 2011; Asevedo et al., 2011; Cozzuol et al., 2011; Mothé et al., 2011). Furthermore, whether or not *Haplomastodon* is a junior subjective synonym of *Stegomastodon* is irrelevant to the need for a diagnostic neotype of the oldest valid species of *Haplomastodon*, given the long-standing and extensive use of the generic name *Haplomastodon*.

### Additional references

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